Popper and the Quantum Theory

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1. Introduction

Popper wrote extensively on the quantum theory. In Logik der Forschung(LSD) he devoted a whole chapter to the topic, while the whole of volume 3 of the Postscript to the Logic of Scientific Discovery is devoted to the quantum theory. This volume entitled Quantum Theory and the Schism in Physics (QTSP) incorporates a famous earlier essay, "Quantum Mechanics without "the" "Observer"" (QM). In addition Popper's development of the propensity interpretation of probability was much influenced by his views on the role of probability in quantum theory, and he also wrote an insightful critique of the 1936 paper of Birkhoff and von Neumann on non-distributive quantum logic[1] (BNIQM). [In this paper I will look at some of Popper's arguments in a suitably critical spirit. But he would have applauded this. My great regret is that he cannot respond to this paper with criticisms of my arguments!

1. Thought Experiments

In 1934 Popper published a short note in *Die Naturwissenschaften* entitled 'Zur Kritik der Ungenauigkeitsrelationen' (KU). This was actually Popper's scientific debut. It contained what Popper later described as 'a gross mistake for which I have been deeply sorry and ashamed ever since' (QTSP, 15). Appendix *xii to the 1959 translation of LSD reproduces a letter from Einstein written in 1935 criticizing Popper's proposed thought experiment. Although he tells us in his autobiography (UQ, 92) that he had defended his experiment against von Weizsäcker and Heisenberg, he immediately concurred with Einstein's criticism. Einstein was Popper's great scientific hero, and the example of the successful novel prediction of general relativity was the paradigm example of Popperian corroboration, although as we shall see later, Popper was in fundamental disagreement with Einstein on the questions of determinism in quantum mechanics. Although Popper's understanding of the physics involved in his thought experiment was definitely at fault, as we shall explain shortly, nevertheless there is a definite sense in which Popper had anticipated ~~the~~ an important ingredient in the famous Einstein–Podolsky–Rosen (EPR) paper of 1935. One can even speculate that Einstein was influenced by Popper here[2] (~~For historical details see Jammer~~, 178). Popper himself remarked in a letter to Max Jammer in 1967, with a humility that was more characteristic of Popper in matters of physics than

Figure 1 Insertion on p. 3.

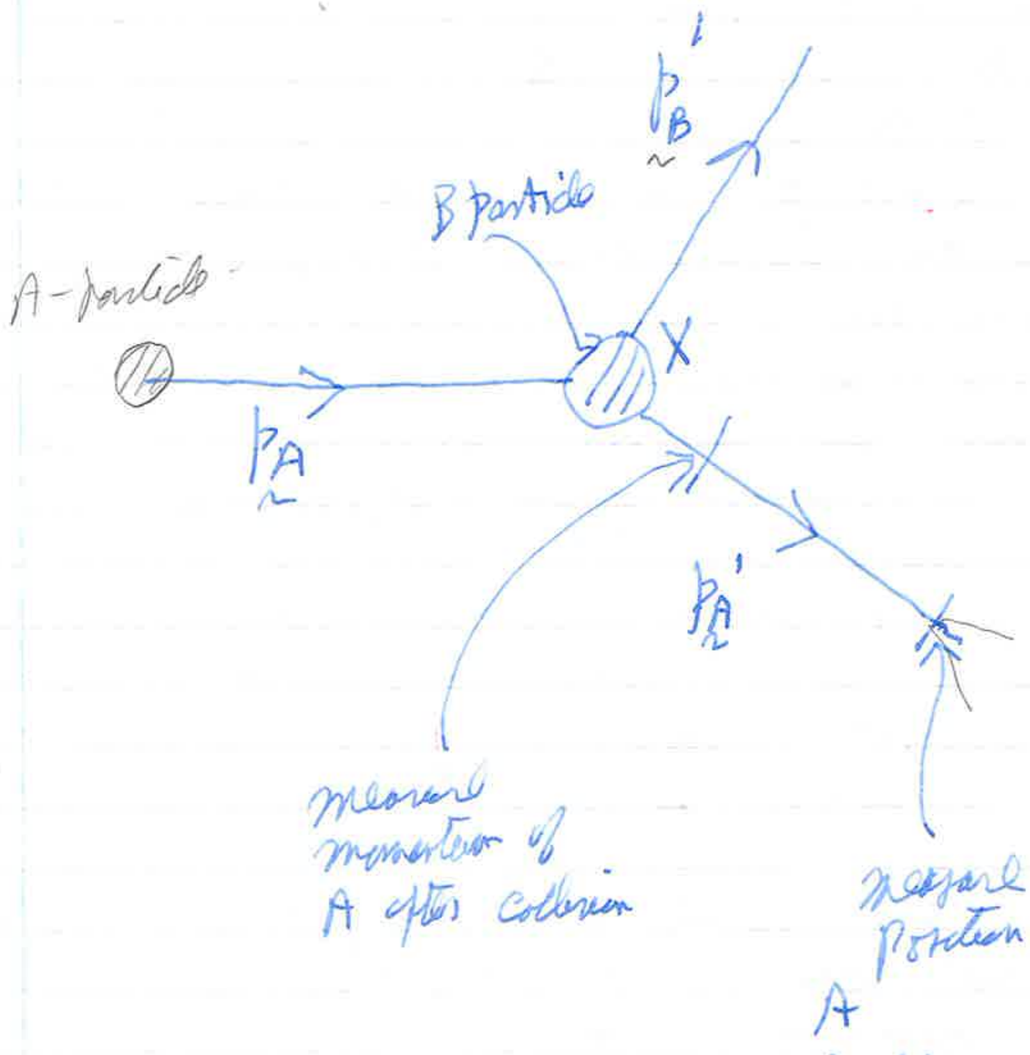
A-particle
$\underset{\sim}{p}_A$
B particle
$\underset{\sim}{p}_B'$
X
$\underset{\sim}{p}_A'$
measure momentum of A after collision
measure position of A

Figure 1 A schematic version of Popper's 1934 thought experiment

in matters of philosophy,' the possibility that a gross mistake made by a nobody (like myself) may have had an influence on a man like Einstein never entered my head'.

I shall now briefly describe the thought experiment, but with some simplifications, that makes the evaluation of the experiment, ~~more~~ I believe, more transparent.
Popper's idea was to consider the scattering of two particles, call them A and B where A has a sharp momentum and B a sharp position. (In Popper's actual experiment the collision is between a parallel beam and a diverging beam. I am simply proposing using the origin of divergence as the point of collision – what Popper refers to as the Schnittpunkt. This greatly simplifies the geometry of the experiment.)

Referring to Fig. 1, A has a sharp incoming momentum $\underset{\sim}{p}_A$ and scatters into a final momentum state $\underset{\sim}{p}_A'$. B has initially a sharp position at the point x and recoils ~~gets scattered~~ into a momentum eigenstate with momentum $\underset{\sim}{p}_B'$. An initially unknown momentum $\underset{\sim}{p}_B$ is absorbed from the B particle, ~~so~~ but this can be computed from conservation of momentum, i.e.

$$\underset{\sim}{p}_B = \underset{\sim}{p}_A' + \underset{\sim}{p}_B' - \underset{\sim}{p}_A \qquad \text{——— (1)}$$

⇒ Insert Figure 1.

Suppose now that we don't know the magnitude of B's final momentum but only its direction, i.e. we don't know $|\underset{\sim}{p}_B'|$.
But this can be calculated from conservation of

energy, if we have measured $|p_A'|$ as well as the final direction of the scattered A particle. Popper now wants to show how to *predict* by measurements on the scattered A particle not only the recoiling *momentum* of the B particle but also its recoiling *position* at any given time. Since the recoil starts at X and we know the momentum and hence velocity of the recoiling particle, (say) as calculated from the measurement of p_A', we can predict the position of the recoiling particle provided we know the time at which the collision occurs. To find this, says Popper, all we need to do is to measure the *position* of the scattered *A* particle ~~after~~ a given time after the momentum measurement, and retrodict from the known velocity of the A particle after the collision, the time at which it must have emerged from X, i.e. the time of collision.
So putting all this together by making successive momentum and position measurements on A, we can predict the momentum and position simultaneously for the recoiling B particle.

The flaw in Popper's argument is that measuring the momentum of the recoiling B particle necessarily affects the possibility of retrodicting its position at still earlier times. For example if we measure the momentum by the Doppler shift of very low frequency radiation, then an accurate measurement of the frequency requires a long wavetrain, i.e. a long duration of time for the irradiation, during which time the average velocity of the B* particle cannot be exactly specified. It is easy to show that this exactly prevents us from retrodicting the position of the B* particle *before* the momentum measurement, and hence prevents us from inferring the time at which the recoil particle emerges from X. An alternative proposal put forward by Popper was to measure the momentum by means of a selective filter. But this again would disturb the position of the recoil particle; for example a wavepacket of precise location incident on the filter which ~~would~~ be indefinitely 'spread out' by the action of the filter in selecting a particular narrow band of wavelengths from the wavepacket.

What EPR do, by contrast, is to devise an experiment in which by measuring *either* ~~p~~ momentum *or* position on ~~the~~ one particle the corresponding momentum or position can be inferred for the other particle. But since both these measurements cannot be made simultaneously, we cannot *predict simultaneous values* for the momentum *and* position of the other particle. If we were able to do this a conceptual selection of a 'super-pure' ensemble of the second particle would be possible, with no dispersion in momentum or position, but this would contradict the objective scatter relations, that Popper regards as the correct interpretation of Heisenberg's uncertainty principle. In LSP, ~~301~~ 245-246 and 301, Popper tries to argue that his thought experiment is not ruled out by violating the objective scatter relations. But I find its discussion here quite unconvincing, confusing the selection of a sub-ensemble by conditionalizing on a random variable, with the *controllable* selection of a sub-ensemble by predictive selection. So without going into any details of the proferred experiment its purported conclusion actually contradicts a *theorem* of the formalism prohibiting super-pure states.

Popper returned to the question of thought experiments in Appendix *XI of LSD entitled 'On the Use and Misuse of Imaginary Experiments, Especially in Quantum Theory'.

Popper 'warns against ~~the~~ *apologetic use of imaginary experiments'* (LSD, 443, Popper's italics). Great care must be taken not to introduce idealizations or other special assumptions unless they are favourable to an opponent, or such that any opponent would have to accept them (LSD, 444).

It is not clear that Popper heeded his own warnings in these matters as will become clear as we proceed to describe the next thought experiment that he devised in order to disprove the orthodox Copenhagen interpretation of quantum mechanics.

In 1982, in the new introduction to Volume 3 of the *Postscript to the Logic of Scientific Discovery* Popper describes what he ~~describes~~ takes as an 'extension of the EPR argument' (~~LSD~~ QTSP, 27 ff.).

The idea behind the experiment 'was to test whether knowledge alone is sufficient to create "uncertainty" and, with it scatter (as is contended under the Copenhagen interpretation), or whether it is the physical situation that is responsible for the scatter.'

Referring to Fig. 2. , Popper proposes a source S from which pairs of particles are emitted in opposite directions in an EPR state, i.e. such that the momenta and the positions of the particles are correlated. The correlated particles are selected through slits A and B, which are sufficiently wide that there is no appreciable diffraction as they pass through the slits, and then fire off, in coincidence, Geiger counters arranged in a semicircle behind the slits, the counters firing between b_1 and b_2 ~~on a~~ behind the B slit and between a_1 and a_2 behind the A slit as indicated in the figure. For simplicity we just consider particles moving in the x-y plane of the diagram.

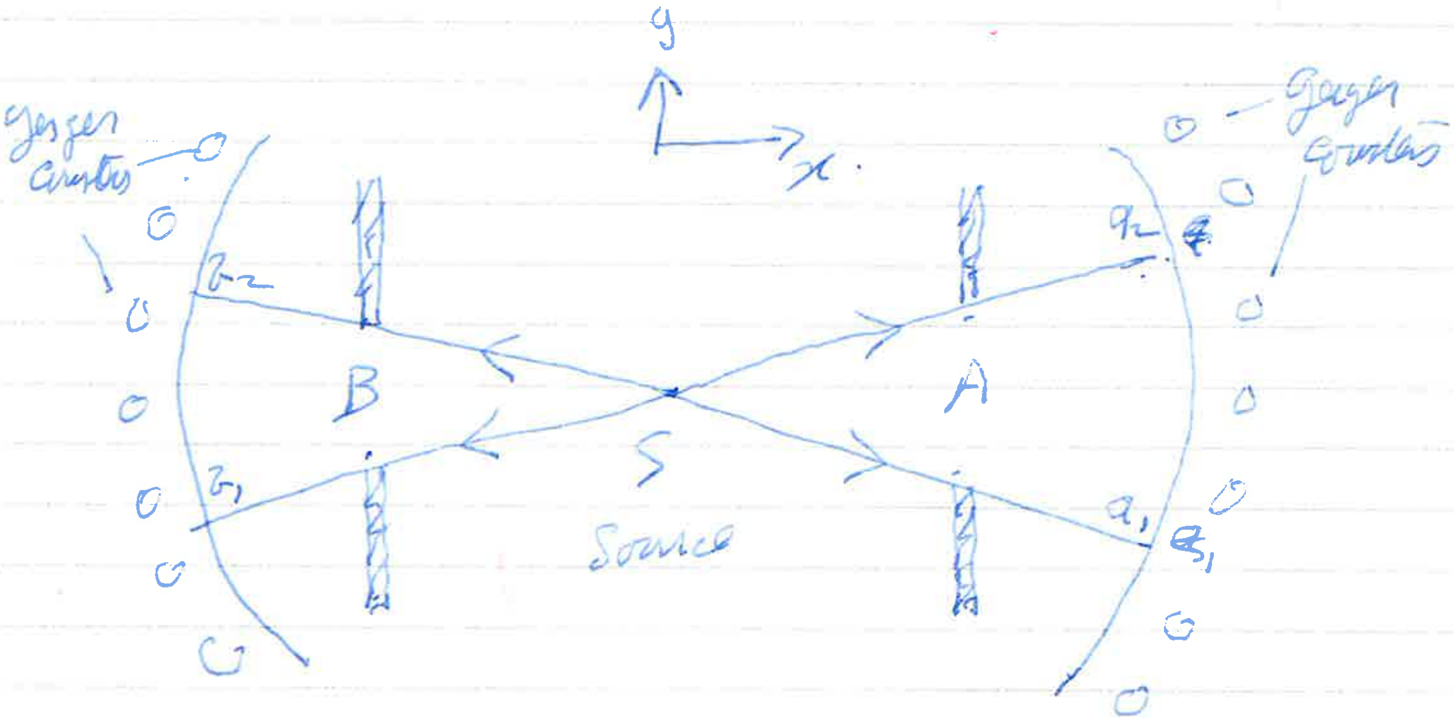

Figure 2 Popper's 1982 Thought Experiment

Since the momenta are correlated, b_1 fires with a_2, and b_2 with a_1, and so on. Now Popper proposes 'narrowing' the A slit as illustrated in Fig. 3. Since the A slit is effectively constraining the y-coordinate of the particles passing through it ~~and~~ then as a result of the position correlation, the y-coordinate of the particle passing through B is also more tightly constrained as we narrow the A slit, and then according to the Copenhagen interpretation the more accurate knowledge of the y-coordinate of the particle passing through the wide B slit will produce a scatter in the transverse momentum, so counters over the wider arc b_1' to b_2' will now fire. (Of course there will be a corresponding scatter behind the narrow A slit).

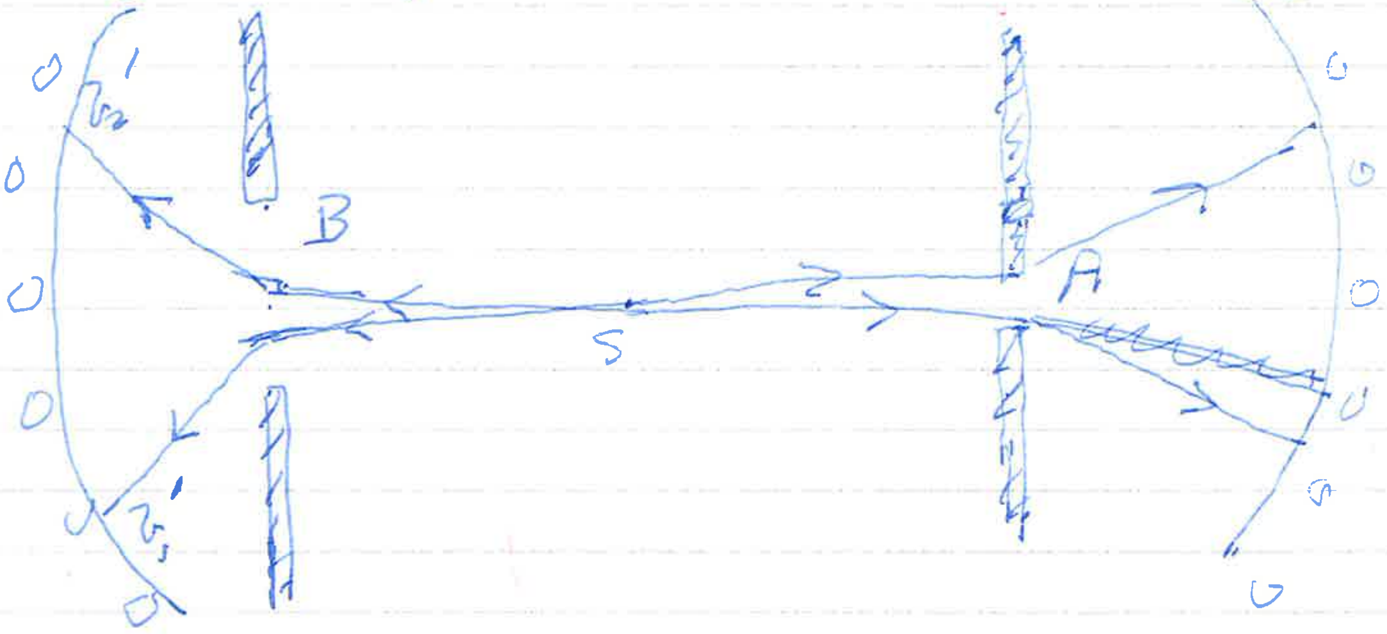

Figure 3 The effect of narrowing the A slit according to Popper is to produce wide-angle scatter behind the wide slit B

Popper is convinced that this effect would not happen, so if performed the experiment would according to Popper, constitute a decisive refutation of the Copenhagen interpretation.

The flaw in Popper's argument[3] is that he misunderstands the nature of the EPR correlations.

The EPR state at the time $t=0$ of emission of the two particles, in respect of the transverse y-dimension is of the form

$$|\Psi_0\rangle = \int_{-\infty}^{\infty} |y\rangle \otimes |y\rangle \, dy \qquad (2)$$

$$= \int_{-\infty}^{\infty} |p_y\rangle \otimes |-p_y\rangle \, d\,p_y \qquad (3)$$

~~The~~ ~~fo~~ The form (2) demonstrates the position correlation and the form (3) the momentum correlation.

So the EPR source, according to (2) is not a point source, like S in Figs. 2 and [illegible] but an infinite incoherent line source. Why incoherent? Because for any observable A_1 of particle 1, for example, the expectation value at time t is

$$\langle\Psi_t|A_1|\Psi_t\rangle = \int_{-\infty}^{\infty} \langle y_t|A_1|y_t\rangle \, dy \qquad (4)$$

where $|y_t\rangle$ is the time-evolved state at time t which starts at $t=0$ as an eigenstate $|y\rangle$ of position.

Similarly for observables referring to particle 2, so expectation values are additive – this is interference between Schrödinger waves originating at different values of y.

So Popper's experiment with an EPR source looks like Figs. 4 and 5 showing wide angle firing of the counters behind the B slit when the A slit is wide, just the same as when it is narrow, i.e. altering the width of the A slit has no effect on which of the counters behind the B slit fire. So the 'Popper effect' is not predicted according to the formalism of quantum mechanics and hence not predicted according to any 'interpretation' of that formalism, in particular, it is *not* predicted by the Copenhagen interpretation.

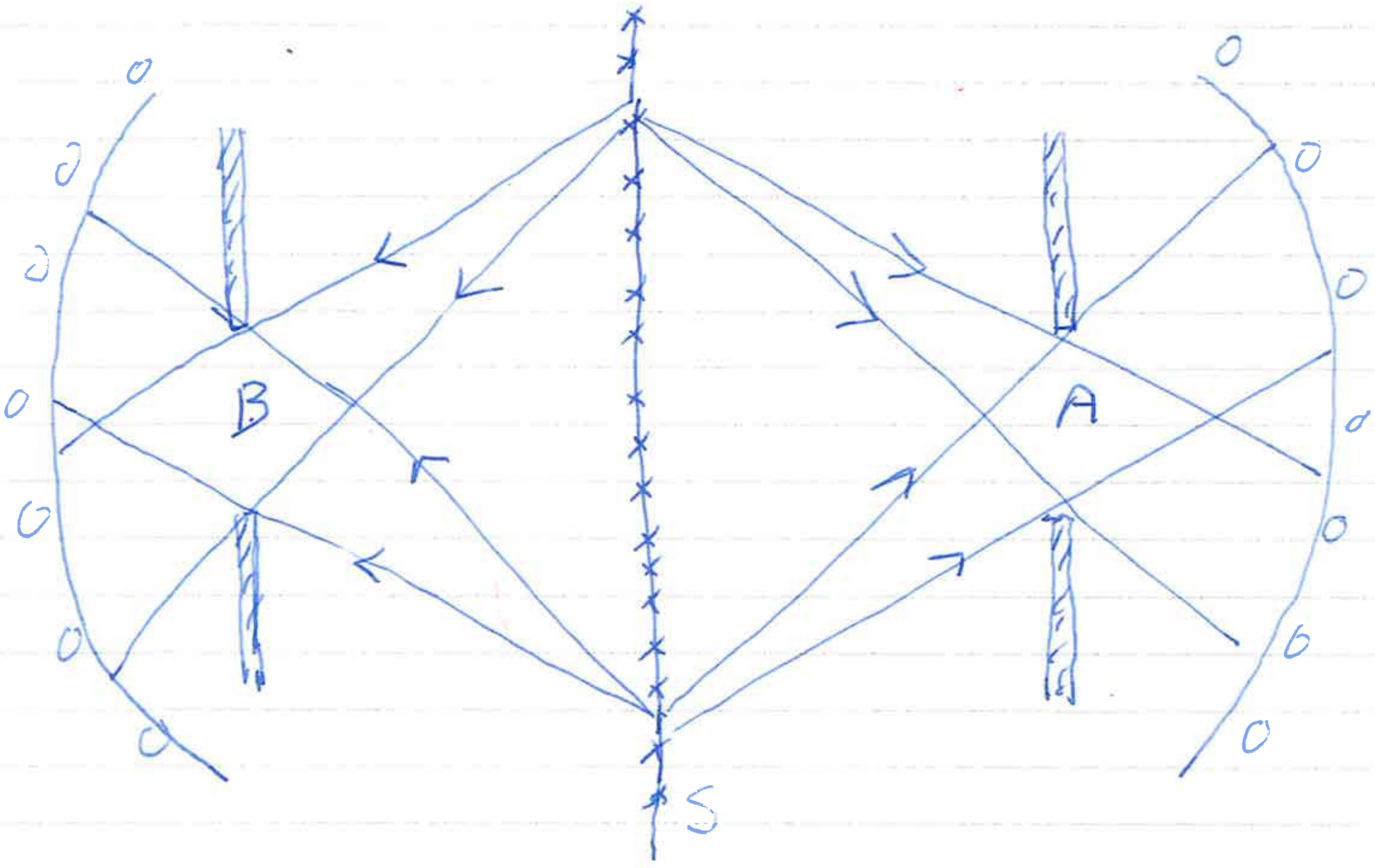

Figure 4 The EPR source with wide slits.

All counters fire on both sides of the experiment

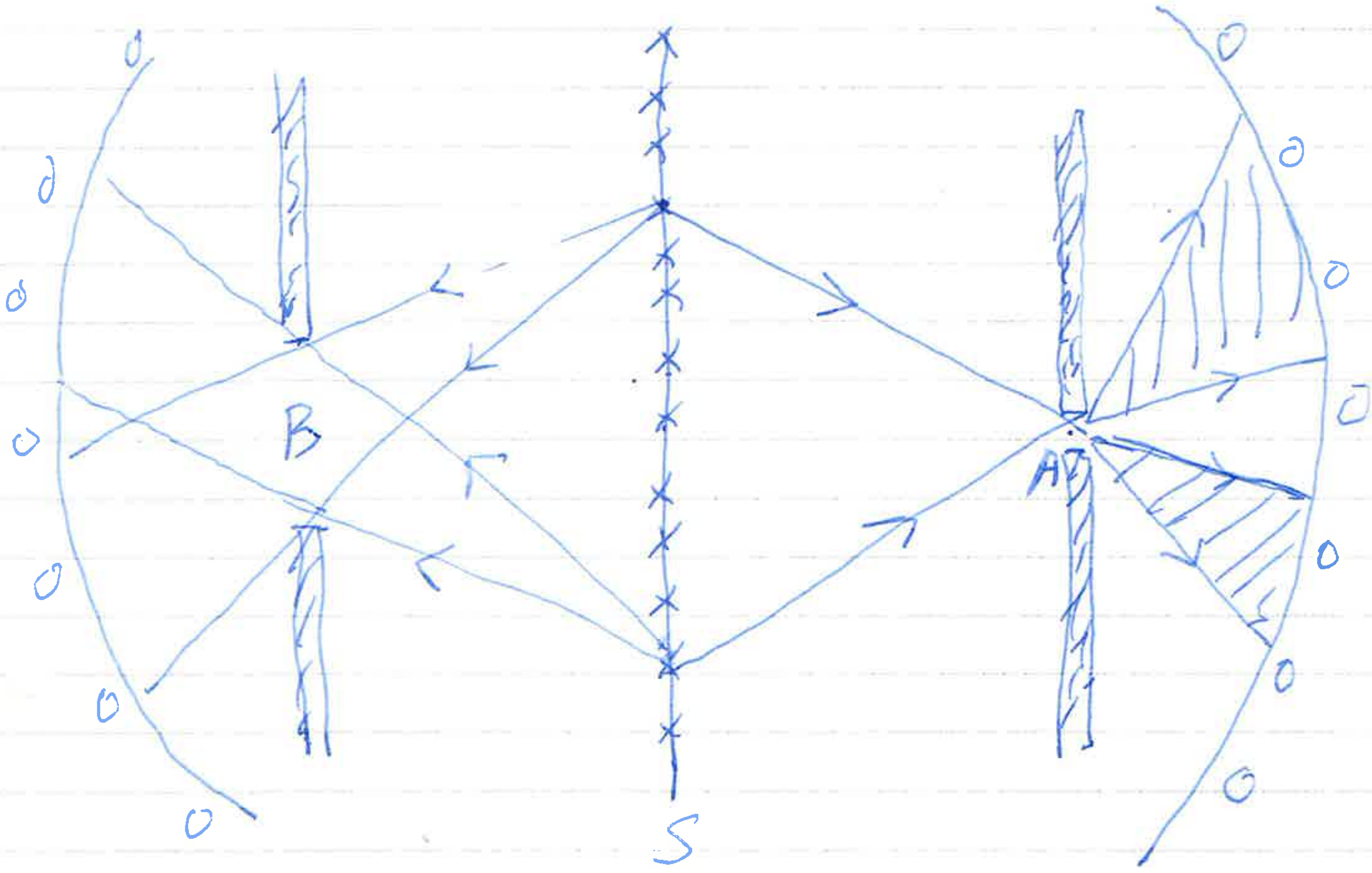

Figure 5 EPR source with a narrow slit at A. Notice diffraction effects behind A, but *all* counters fire on both sides of the experiment.

When I showed these diagrams to Popper, he refused to accept that I had a general argument that narrowing slit A could not make a counter fire behind B that would not have fired when A was wide. Perhaps by altering the geometry he could get what he wanted. To get rid of the wide-angle firing behind B in *all* circumstances it is ~~now~~ obviously necessary to truncate the line source. But truncating the EPR source by replacing the infinite limits in Eqn. (2) by finite limits, then we lose the momentum correlation, i.e. we can no longer rewrite (2) in the form (3). In other words, with a truncated source, a narrow beam on the right in Fig. 3 does not correlate with a narrow beam on the left, with resulting diffraction as Popper supposed. Fig. 3 should actually be replaced by Fig. 6 showing no extra diffraction ~~between~~ behind B, as compared with the situation with both slits wide as illustrated in Fig. 2.

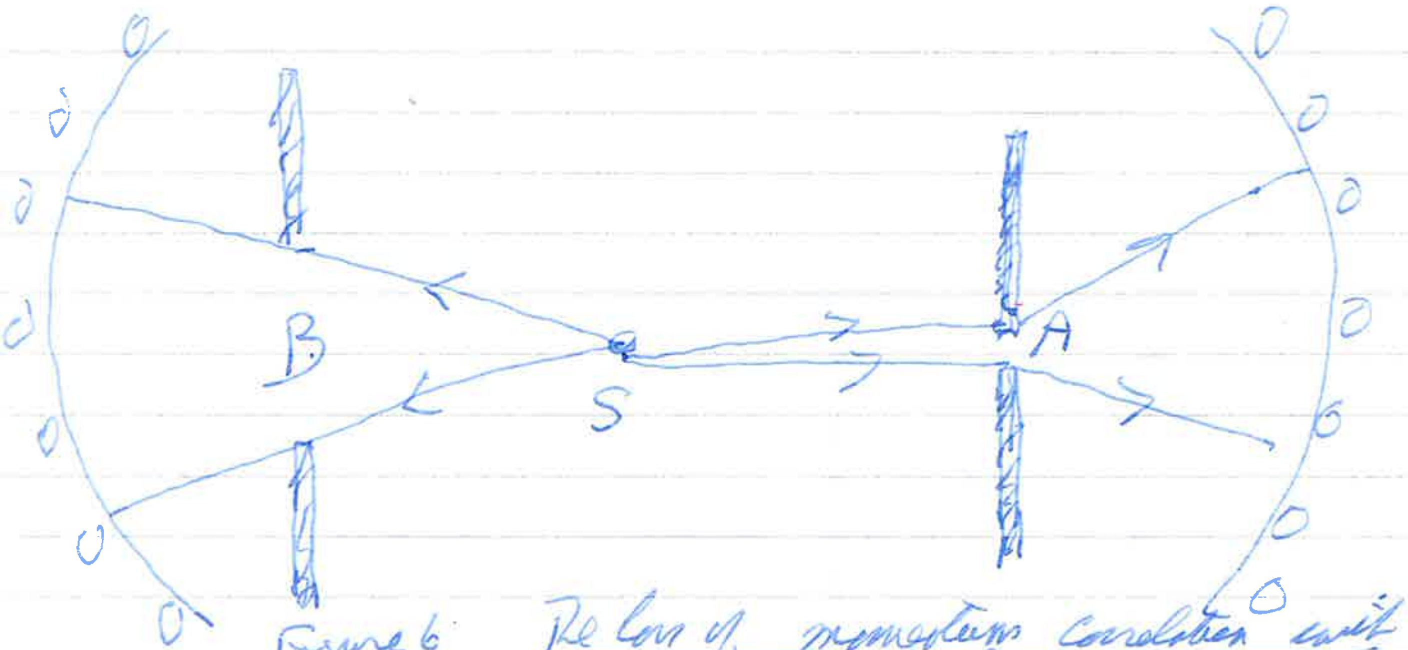

Figure 6. The loss of momentum correlation with ~~a point source~~ localized source

But Popper was still not convinced! So I provided the following general proof. Consider any observable A for the particle passing through the A slit with eigenvalues a_i, and similarly for B on the left with eigenvalues b_j.

Then since A and B commute we write, in an arbitrary state Ψ

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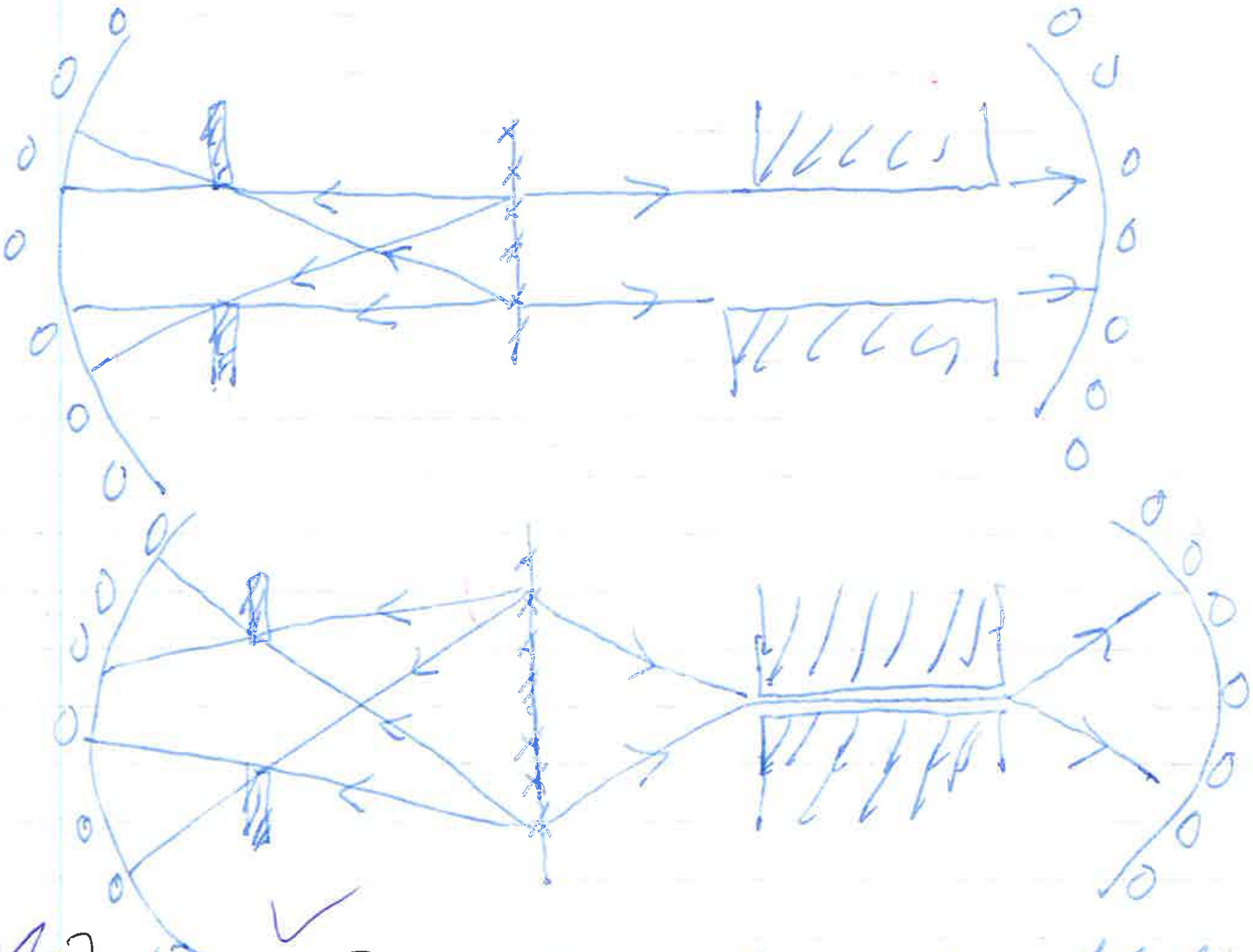

16/?

Figure 7 The Doppler effect and a thick slit

$$\mathrm{Prob}^{\Psi}(b_j) = \sum_i \mathrm{Prob}^{\Psi}(b_j|a_i) \times \mathrm{Prob}^{\Psi}(a_i) \qquad (5)$$

Now suppose $\mathrm{Prob}^{\Psi}(b_j=0)$ and $\mathrm{Prob}^{\Psi}(a_i) \neq 0$ for some particular index i. Then (5) implies that under these conditions $\mathrm{Prob}^{\Psi}(b_j|a_i) = 0$, i.e. selecting a sub-ensemble on the left by conditionalizing on the right with an event that has a non-zero probability of occurring cannot convert a zero probability for b_j into a non-zero probability for b_j.

By a curious irony it is possible to produce the 'Popper effect', but not in the way Popper thought, by using a thick A slit. The effect is illustrated in Fig. 7.

6a?

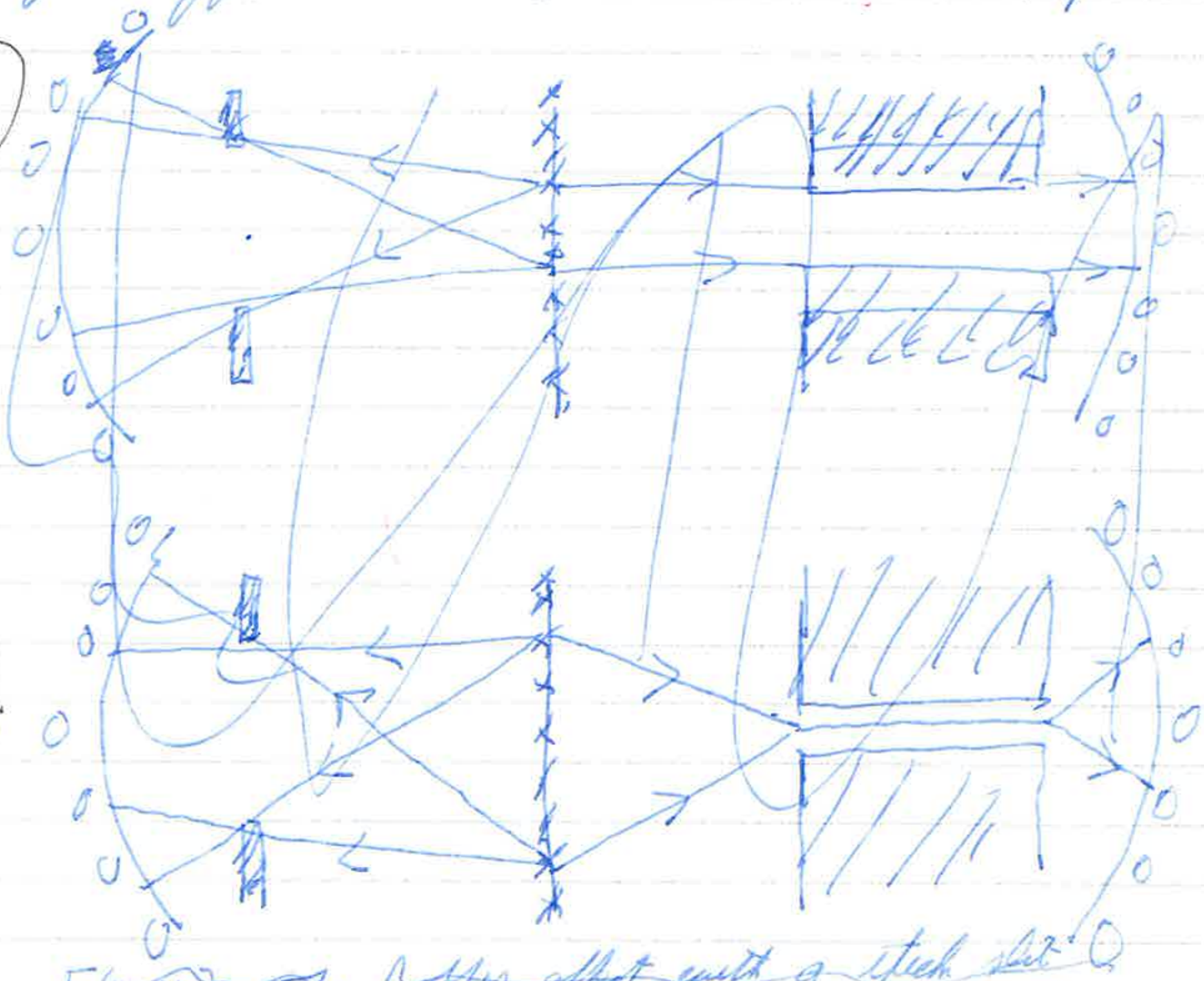

Figure 7 The Popper effect with a thick slit

⇒ Insert Figure 7.

6?

where we suppose the thickness of the A slit is very long compared with its width.

The increased angle of firing behind the A slit is now produced by the fact that due to diffraction effects at the narrow A slit we can actually produce, not a sub-ensemble, but a larger ensemble of ~~source~~ points on the line source as compared with the case of the wide ~~slit~~ slit.

2.3 Indeterminism and Propensities

Popper's approach to the quantum theory is based on two key ideas:

a) The prevalence of indeterminism in physics. Instead of the slogan 'all clouds are clocks', Popper wants to substitute 'all clocks are clouds' (OK, 215). His arguments for indeterminism apply in classical physics just as much as in quantum physics. They are based, for example, on issues of instabilities ~~that~~ arise in classical mechanics, on logical paradoxes produced by attempts at self-prediction and the pervasive operation of what Popper calls the Oedipus-effect (OU, 66) whereby the very fact of making a prediction can influence the predicted event.

There are ~~good~~ criticisms of scientific determinism, roughly speaking the possibility of a Laplacean demon being able to 'predict the future'. But Popper also attacks what he calls metaphysical determinism, the view, picturesquely, that the future is 'coiled up' in the present. For Popper this denies the reality of time and the possibility of real novelty 'arising' in the Universe. Popper here strongly opposed Einstein who defended determinism ('God does not play dice ...'). Describing a ~~the~~ conversation with Einstein a Princeton in 1950, he ~~refered~~ refers to Einstein as ~~the new Parmenides~~. Einstein – Parmenides (UQ, 130). Popper's real motivation for his detestation of determinism ~~lie~~ lay perhaps in his belief in human freedom, the unpredictability of a Mozart symphony or a Beethoven sonata, the development of new arguments and theories, the denizens of his World 3, that creative evolution is outside the framework of World 1 alone and that World 1 is crucially affected by World 3, via the mediation of World 2, and so on.

B) For these reasons Popper thinks that probabilities in physics cannot, in general, be epistemic. How could human ignorance produce genuine physical effects? Popper originally held a frequency view of probability but the explanation of stable limiting frequencies (i.e. of random sequences) was for Popper a physical fact that needed explaining via an objective notion of tendency, and this led him in the 1950's to his propensity interpretation. This he regarded as particularly well suited to explaining the scatter relations which he used for interpreting the Uncertainty relations in quantum theory.

3.4. State Preparation and Measurement

Popper regarded the Heisenberg Uncertainty relations as limitations on the possibility of producing homogeneous ensembles with precise values of position x and momentum p, for example. So Heisenberg had discovered a limitation on state preparation, not a limitation on measurement. 'When we measure x or p this is a backward looking exercise as contrasted with the forward looking exercise of state preparation, and such measurements are necessary to 'test' the ~~scatter rel~~ reciprocal scatter relation expressed in the Heisenberg ~~relation~~ principle.

Moreover, Popper says ~~that~~ he believes that between interactions particles behave classically, i.e. following Newtonian straight-line trajectories, but when interactions occur they get disturbed in an ~~in an~~ intrinsically statistic stochastic fashion and this is what produces the quantum-mechanical scatter.

As an analogy Popper often referred to what he calls the pin board (i.e. a form of bagatelle – cf. Q.9, Q.T.S.P., 72f, 87, 153). Here the balls roll deterministically between impacts with the pins. The statistical distribution of the balls at the bottom of the board is the manifestation of a propensity to produce individual results, the final position of each individual rolling ball. The propensity is a property of the whole ~~set~~ set-up of pins and balls and is not to be thought of as property of the individual balls themselves. This, for Popper, is the origin of what he calls 'the great quantum muddle' (QTSP, 52). The mistake here is like treating the average height of people in a room as a property of each particular person in the room. My own view is that it is perfectly legitimate to regard the quantum-mechanical propensities as properties of the particles, properties manifested in the context of particular experimental arrangements. This means of course that there are as many properties as there are experimental arrangements, so, following Popper, it may sound convenient to 'talk' of 'the' property as belonging to the experimental arrangement but it does not seem to me mandatory to 'talk' in this way ~~(cp Redhead (1987, 49)~~.

Returning to Popper and the pin ~~table~~ board, changing the arrangement of the pins changes the propensity — indeed the arrangement of the pins act like a sort of stochastic field of ~~[illegible]~~ force influencing the individual balls. Popper sees this as explaining, in a general sort of way, the two-slit experiment. The closing of a slit is like removing a pin from the pin board. Even if the ball on a 'particular' occasion never hits the pin nevertheless the propensity changes. Popper, of course stresses (in response to a critique of Feyerabend[4]) that the pin board does not exhibit the characteristic interference phenomenon of the two-slit experiment. To explain that is a matter for the physics of the relevant propensities. Popper generally believes in the Landé approach[5], that the effect of the two slits on the beam of electrons is related to the periodicity of the structure, i.e. the separation of the slits which controls the transfer of momentum to the electron as it passes through one of the slits. If we close a slit we change the sort of object that the electron is interacting with, and hence the character of the interaction.

Turning to another aspect of measurement the famous (or infamous) collapse of the wave function, a projection postulate. this Popper regards as a quite unproblematic consequence of conditionalizing a probability distribution on the outcome of an experiment (QTSP, 78). Consider a coin toss. When the coin lands heads, the probability of heads jumps from $1/2$ to 1, and the collapse of the wave function is just an instance of this type of behaviour.

Let us look at this from the point of view of joint distributions. In standard probability theory one defines conditional probabilities in terms of joint distributions, thus $\mathrm{Prob}(A/B) \overset{=}{_{DF}} \mathrm{Prob}(A \,\&\, B)/\mathrm{Prob}(B)$, provided $\mathrm{Prob}(B) \neq 0$. Popper's own approach to probability theory starts with conditional probabilities as basic and proceeds to joint distributions [illegible] defining ~~$\mathrm{Prob}(A \,\&\, B) \overset{=}{_{DF}} \mathrm{Prob}(A/B).\mathrm{Prob}(B)$~~

$\mathrm{Prob}(A \,\&\, B/C) \overset{=}{_{DF}} \mathrm{Prob}(A/B \,\&\, C).\mathrm{Prob}(B/C)$

(cp. LSD, 332).

But either way of proceeding seems committed to joint distributions such as $\mathrm{Prob}(\underline{x} \,\&\, \underline{p})$ (perhaps conditionalized on an experimental set-up) for incompatible observables such as $\underline{x}$ and $\underline{p}$. So what is the status of joint distributions and simultaneous

measurability in quantum mechanics?
Suppose we measure an observable Q and find the value q, so the new state (according to the projection postulate) is the eigenket |q⟩.
If now we measure an incompatible observable Q' and find the value q', then it seems reasonable to claim that for the state |q⟩ Q has the value q and Q' the value q'.
Of course we cannot use this knowledge to predict the value of Q *after* we measured Q'. This might well differ from q, due to the disturbing effect of the measurement of Q'.

But can we measure Q and Q' when the state |ψ⟩ is not an eigenstate of Q (or Q').
A typical proposal here is to measure an observable, call it U which is *compatible* with Q and has the same numerical eigenvalues as Q', and in the state |ψ⟩, has the same 'probability distribution' as Q'. So U and Q' are matched probabilistically but it does not follow that on a particular occasion the result for U is the same as the result we would have found if we had measured Q'.
The classic example of this is the time-of-flight measurement of momentum. [we consider a non-dispersive medium.] An initial measurement of position locates a particle 'sharply' at the origin at time t=0, then if we measure the position X again at a later time t, the quantity $U = \frac{mX}{t}$ is distributed 'probabilistically' in the same way as the momentum scatter generated by the initial position measurement.
But measuring X allows us to infer the value of U, so does the combination of values x for X and u for U (where $u = \frac{mx}{t}$) constitute a

joint measurement of position and momentum for the particle: The question is, does the V measurement give the same result, on a particular occasion as a 'direct' measurement of momentum e.g. by bending in a magnetic field, or using the Doppler shift to measure the velocity and hence the momentum. Note that for all these direct measurements, assuming they are of the so-called first kind, is that the properties postulate affirm, the results of one procedure can be checked by applying the other procedures, since they are all compatible procedures. But in the case of the time-of-flight approach, after the x measurement the momentum is disturbed, so the value before the x measurement cannot be checked by a subsequent direct measurement.

Nevertheless, if we assume uniform rectilinear trajectories for the particle after its initial position measurement at $t = 0$ as Popper does, then V must be measuring the momentum that during the interval between the two position measurements, although as we have seen this is something that cannot be checked or tested.

Now what does all this tell us about ~~the~~ correct joint distribution for x and p. Clearly ~~it is~~ the probably joint density is of the form $\delta(p - \frac{mx}{t}) \cdot \text{Prob}(x)$, which certainly returns the right marginals for x and p. But this only works for a particular state. Indeed joint distributions can always be defined for particular states e.g. take the joint density to be of the form $\text{Prob}(p) \times \text{Prob}(x)$. Again this certainly returns the right marginals. But if we ask the

question, is there a quantum-mechanical observable, that measures the joint quantity 'x and p' and returns the right marginals in all states? The answer is 'no', according to an important theorem of Fine[6] ~~([illegible])~~.
Constructions in the literature[7] (~~see, e.g.~~ Wigner (1932)) that do return the right marginals in all states, do not give non-negative values for the joint distribution and so these do not qualify as bona fide probabilities.
So it is difficult to fit joint distributions ~~into the form~~ for incompatible observables into the formalism of quantum mechanics and indeed Arthur Fine ~~he~~ has urged that we should reject them, even in a phase-space reconstruction of quantum mechanics. As Fine has argued, joint distributions in probability theory only arise in general when x and p, for example, are regarded as random variables over an underlying probability space, and we don't have to make this overly strong assumption[8] ~~(see Fine (1968))~~.
It would have been interesting to know Popper's views on this question. As far as I know he never referred to Fine's arguments on this point.

4. Nonlocality and Bell's Inequality

Another important issue in the philosophy of quantum mechanics, in the kind of realist construal espoused by Popper, is the question of proofs of nonlocality via the theoretically predicted and experimentally verified violation of the famous Bell inequality.

Popper made two sorts of comment here. Firstly, if the experiments and their interpretation were taken at face value, then he regarded the Aspect experiment for example, as a crucial test of the Lorentz versus the Einsteinian interpretation of the relativity transformation equations. Instantaneous action would call for a privileged reference frame, an aether frame if you like.

But secondly, Popper also leaned to the view that the theoretical proofs of the impossibility of local hidden-variable theories for experiments such as Aspect's might contain technical flaws. He gave support to the work of Thomas Angelidis[9] who sought to demonstrate local models of these experiments. My last correspondence with Popper, just before his death, was concerned with the validity of this work. In my view the models were not correctly described as local. Popper himself admitted that the whole matter was too technical for him to fully check everything, but urged that the latest work of Angelidis should be fully aired in the literature. This is something I propose to do on a future occasion.

At first sight it might appear that, with

Greater Indeed, he was critical of Bohm's
hidden-variable approach for ~~other ways~~
Just reason because of Bohm's commitment
~~to~~ determinism

Popper's emphasis on indeterminism, he should be careful in examining ~~pursue~~ the stochastic hidden-variable approach[10] (~~see, e.g. Redhead (1989), 98 ff.~~) *

But in doing this he is really being inconsistent with his other views. For his commitment to deterministic behaviour between interactions coupled with his adherence to a theory of faithful measurement, means that the measurement outcomes are deterministically related to the state of the particles after they have left the source. Hence the deterministic hidden-variable proofs of the Bell inequality are the ones ~~relevant to~~ that need to be examined ~~by Popper~~, consistently with the views he espoused in other contexts

5. Conclusion

Popper showed (QTSP, 6) that 'the strongest reason for my own opposition to the Copenhagen interpretation lies in its claim to finality and completeness'. I entirely agree with the view that we should not rule out criticism by fiat or authority. Popper fought a lone battle against the Copenhagen interpretation at a time when anyone attempting to criticize orthodoxy was liable to be labelled at best an 'outsider' or at worst a crank. But Popper's carefully argued criticisms won the support of a number of admiring and sophisticated physicists. He has done a great service to the philosophy of quantum mechanics by emphasizing the distinction between state preparation and measurement and trying to get a clearer understanding of the true significance of the uncertainty principle, ~~but~~ and above all by spearheading the resistance to the dogmatic tranquilizing philosophy of the Copenhageners. Because some detailed arguments are flawed, this does not mean that his overall influence has not been abundantly beneficial.

Notes (to be inserted ~~in the text~~ at the foot of the relevant page).

1. In this paper Popper ~~claimed~~ to exploited some ambiguity in the Birkhoff and von Neumann paper concerning the distinction between complement and orthocomplement in a lattice, and by making a plausible measure-theoretic assumption concerning their lattice showed that it was actually distributive rather than nondistributive. For a detailed crit critique of Popper's BNICM reference may be made to F. Scheibe 'Popper and Quantum Logic', *The British Journal for the Philosophy of Science* 25 (1974) 319-328.

2. For historical details see M. Jammer, *The Philosophy of Quantum Mechanics* (Wiley, New York, 1974), p. 178.

3. There are other critiques of the experiment in the literature making broadly similar points to my own. See, for example, A. Sudbery, 'Popper's Variant of the EPR Experiment Does Not Test the Copenhagen Interpretation', *Philosophy of Science* 52 (1985) 470-476. and H. Krips 'Popper, Propensities, and Quantum Theory', *The British Journal for the Philosophy of Science* 35 (1984) 253-282.

4. P. Feyerabend, 'On a Recent Critique of Complementarity', *Philosophy of Science* 35 (1968) 309-331; 36 (1969) 82-105.

5. A. Landé, *Quantum Mechanics* (Pitman, London, 1951)

6. A. Fine, 'Probability and the Interpretation of Quantum Mechanics', *The British Journal for the Philosophy of Science* 24 (1973) 1-37.

7. See for example E. Wigner, 'On the Quantum Correction for Thermodynamic Equilibrium', *Physical Review* 40 (1932) 749-759.

8 See A. Fine, 'Logic, Probability, and Quantum Theory', *Philosophy of Science* 35 (1968) 101-111.

9. J. Angelidis, 'Bell's Theorem: Does the Clauser-Horne Inequality Hold for all Local Theories?' *Physical Review Letters* 51 (1983) 1819-1822. and 'On the Problem of a Local Extension of the Quantum Formalism', *Journal of Mathematical Physics* 34 (1993) 1635-1653. The first paper has been criticised by a number of people. ~~But~~ The second paper has not so far attracted critical comment, at all in the literature.

10 See, for example M. L. G. Redhead, *Incompleteness, ~~Noncolo~~ Nonlocality, and Realism* (Clarendon Press, Oxford, 1987), p. 98 ff.